A TREATISE ON THE INFLUENCE OF THE MOON IN FEVERS.

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TO THE

HON. WARREN HASTINGS, Eſq;

GOVERNOR GENERAL, &c.

SIR,

THE flattering approbation you beſtowed on my Manuſcript, and the deſire you expreſſed to have it publiſhed, left me no room for heſitation.

As it aſpires at improvements that are important to mankind, this ſmall ſpecimen of my induſtry had reaſon to hope for your attention on the grounds of its apparent utility. And if there appear in its dreſs, any portion of the order and becoming attire of Science, I will own that I have been anxious to give it that form, from a perſuaſion that it would not paſs undiſcerned, or unapproved.

SIR,

I have the honour to be,

With great reſpect,

Your moſt obedient,

And moſt humble ſervant,

FRANCIS BALFOUR.

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PRE-

PREFACE.

THE influence of the heavenly bodies over the human frame has not eſcaped the notice either of the ancients or moderns; but their obſervations on this ſubject have not eſtabliſhed any ſyſtem or rule of certain or extenſive uſe in the practice of medicine. The former were unenlightened by the improvements of our times, in natural philoſophy and aſtronomy; and the clouded and unſettled climates of the latter, ſeem to have denied them that conſtant and uniform ſucceſſion of ſimilar appearances, that is neceſſary to impreſs the idea of a general law.

The reſidence of more than fourteen years in a country where, during eight months of the twelve, ſcarcely a drop of rain falls, or a cloud obſcures the ſky; and where the influence of the moon ſeems to ſhow itſelf in an uncommon degree; has given me an opportunity of obſerving this influence in ſo great a number of caſes, and with ſo little variation, that for many years paſt it has been firmly eſtabliſhed in my mind, as a fixed principle, which has directed my practice on every occaſion.

In this treatiſe I have confined my obſervations to the following propoſitions concerning Fevers.

I. THAT, in Bengal, fevers of every denomination are, in a remarkable manner, connected with, and affected by, the revolutions of the moon.

II. THAT,

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II. That, in Bengal, a conſtant and particular attention to the revolutions of the moon, is of the greateſt importance in the cure and prevention of fevers.

III. That the influence of the moon in fevers, prevails, in a ſimilar manner, in every inhabited part of the globe; and, conſequently, that a ſimilar attention to it is a matter of general importance in the practice of medicine.

IV. That the whole doctrine of the Criſis of Fevers may be readily explained, from the premiſes eſtabliſhed reſpecting the influence of the moon, in theſe diſorders, at the full and change.

No ſubject in medicine has occaſioned greater diviſion than the theory of fevers; and none, conſidering their frequency and danger, is of ſo much importance. If, therefore, in the courſe of theſe diſſertations,

tions, I have hit on a line of accommodation between learned and ingenious men of different opinions; or if I have contributed to unfold a principle, by which the errors of antiquity may be rejected, and the ſucceſsful practice of modern times eſtabliſhed on a ſyſtem that is conſiſtent, and capable of demonſtration; I ſhall conſider myſelf ſingularly fortunate.

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ON THE INFLUENCE OF THE MOON IN FEVERS.

PROPOSITION I.

IN BENGAL (*a*), FEVERS OF EVERY DENOMINATION ARE, IN A REMARKABLE MANNER, CONNECTED WITH, AND AFFECTED BY, THE REVOLUTIONS OF THE MOON.

THE bilious intermittent fever, which appears for the moſt part in the form of a tertian, or of a quotidian, and ſeldom in that of a quartan, is by far the moſt common fever in this country. In whatever form it preſents itſelf, I have almoſt invariably obſerved, that its firſt attack is on one of the three days which imme-

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(*a*) Under Bengal I comprehend all the poſſeſſions of the Company in this quarter of India, with the dominions of the Vizier.

immediately precede the full of the moon, or on one of the three days which immediately follow it; or on one of the three days which immediately precede and follow the change of the moon. I have obſerved the remarkable connection which prevails at this time evidently, at leaſt three complete days both before and after the full and change of the moon. So that it continues at leaſt ſix complete days at each. In general, I think that the days of the full and change are more powerful than any other, and thoſe that follow the full and change more powerful than thoſe that precede: but my obſervations reſpecting this point do not allow me to ſpeak with any confidence. I ſhall, therefore, when I have occaſion to mention the full and change of the moon, comprehend in my meaning the whole of the ſix days already deſcribed, at each of theſe periods, without regard to any one in particular. With reſpect to theſe two periods, I cannot poſitively ſay which of them has the greateſt power of producing fever.

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The full and change of the moon are no leſs remarkable for occaſioning relapſes than for inducing the firſt attack of bilious fevers. This is a fact ſo well eſtabliſhed, that there are few Europeans who have reſided for any time in this country, who are not ſufficiently informed of it, either from their own perſonal experience, or from the daily proofs of it that occur in the circle of their acquaintances. But it cannot poſſibly eſcape the notice of any perſon who practiſes medicine with the ſmalleſt attention for a few months. For my own part, I have obſerved this tendency to relapſe at the full and change invariably for theſe fourteen years; and in particular caſes can prognoſticate the return of the fever, at theſe periods, with almoſt as much confidence as I can foretel the revolution itſelf.

The detached examples which occur in the courſe of common practice, not being properly attended to, or aſſembled, may ſometimes, perhaps, prove inſufficient for impreſſing the truth of this general obſervation; but whenever a number of ſick are collected together in one place, and a multiplicity of correſponding proofs are repeatedly preſented at the ſame inſtant, perſuaſion follows inſenſibly, and every doubt is removed.

In the years 1773-4 I had for many months the charge of a regiment of Sepoys, in the province of Cooch Behar, immediately under the vaſt range of mountains which ſeparates the northern parts of Bengal from Boutan. The diſorders were chiefly fevers, or fluxes attended with fever; and in the ſpace of the firſt month above 400 men were taken ill. The greateſt part of theſe, however, got quit of the fever in the courſe of the eight days which intervened between the full and change, and, by the aſſiſtance of medicine, were ſoon reduced to 70 or 80. But during the remaining months of our ſtay in that country, the ſick conſtantly increaſed nearly to double this number at every full and change; falling down again as conſtantly to their former ſtandard, during the eight days which intervene between theſe two periods. In future I ſhall call theſe intervening days the Intervals.

Although I am now endeavouring to eſtabliſh the ſuperior influence of the full and change of the moon in producing bilious fevers, I muſt, at the ſame time, allow that the intervals are by no means exempted, either from firſt attacks, or relapſes. But theſe happen much leſs frequently at this time; and when they occur, furniſh arguments to ſupport the propoſition we are trying to prove: for the fits are now leſs ſevere, of ſhorter duration, and yield much more eaſily to medicine, than thoſe which happen during the full and change; and the approach of theſe two periods as certainly increaſes the violence of the diſorders when preſent, as the coming on of the interval brings along with it a remarkable abatement of the ſymptoms, or a perfect ſolution of the fever.

The remittent fevers I have met with in this country have either been purely bilious, or of a putrid tendency, ſuch as the Pucca fever of Bengal, deſcribed by Dr James Lind; or a few others of the ſame nature, leſs rapid in their progreſs, and reſembling more the putrid fevers delineated by Sir John Pringle; to which I muſt add ſome rheumatic and nervous fevers, and likewiſe the fever which accompanies the eruption of the ſmall-pox.

With regard to bilious remittents, whether you conſider the particular time of of their attack and relapſe; the ſeverity and duration of the paroxyſm, and its diſpoſition to remit; or the different changes that take place on their tranſition from the full and change to the intervals, and from the intervals to the full and change; the influence of the moon at theſe periods is no leſs remarkable in them, than it is in intermittents.

The inaugural diſſertation of Doctor James Lind on the putrid fever, which firſt prepared my attention for this ſubject, furniſhes many proofs of the influence of the full and change in this diſorder; to which I can now add my own experience, and the conſent of many other gentlemen who have been employed in practice at Calcutta.

In the few putrid fevers I have ſeen elſewhere, and in a ſmall number of rheumatic and nervous fevers, the influence of the full and change never failed to ſhow itſelf in a remarkable manner; and in every caſe, where I have had an opportunity of making obſervations, I have ſeen it exert itſelf ſtrongly in the fever which accompanies the eruption of the ſmall-pox.

But theſe obſervations are not confined to intermittent and remittent fevers. Head-achs, tooth-achs, inflammations of the eyes, aſthmas, pains and ſwelling of the liver and ſpleen, fluxes, ſpaſms, and obſtructions in the bowels, complaints in the urinary paſſages, eruptions of different kinds, and a great many more, unattended by any obvious fever, aſſume often an intermitting form; and regularly return or increaſe with the full and change of the moon, and diſappear or diminiſh during the intervals.

In general, as far as my experience extends, the attack of intermittents, during the influence of the full and change, happens at ſome period between eight in the morning and ſix in the evening. The acceſſion of the paroxyſm in remittents of all kinds is pretty much limited to the ſame time; and the period at which their remiſſions ſeem to be moſt complete, is between three and eight in the morning. Fevers no doubt attack at every hour of the day and night, but this obſervation I think holds good in the full and change (*b*).

It is from the above analyſis that I would venture to advance the propoſition with which I ſet out in the beginning. I now proceed to ſhow, That "IN BENGAL A "PARTICULAR AND CONSTANT ATTENTION TO THE REVOLUTIONS OF "THE MOON IS OF THE GREATEST IMPORTANCE IN THE CURE AND PRE"VENTION OF FEVERS."

PRO-

(*b*) I have no doubt of the ſuperior influence of the moon in fevers at the time of the equinoxes; but my obſervations have not been accurate, and the revolutions of the planets open a field which I have not attempted to explore.

ON THE

INFLUENCE OF THE MOON IN FEVERS.

PROPOSITION II.

In Bengal a constant and particular attention to the Revolutions of the Moon is of the greatest importance in the Cure and Prevention of Fevers.

ALTHO' it be no part of my preſent intention to enter into a particular detail of my practice, but ſolely to eſtabliſh a ſingle principle by which it is greatly directed, I find it previouſly neceſſary to deliver my ſentiments with regard to the Bark.

Having practiſed in Calcutta in 1769 and 1770, during the ſeaſon in which the remittent fever of Bengal, commonly called the Pucca Fever, prevailed, I communicated my obſervations on this ſubject to Sir John Pringle, in 1772. In that Treatiſe I related my practice at large, and drew from it the following concluſions; which I have had no reaſon ſince to retract.

1ſt, That the bark (*c*) in powder is a certain cure for the putrid remittent of Bengal, commonly called the Pucca Fever.

2dly, That there is no ſymptom whatever that ought to prevent it from being exhibited, after the bile is duly evacuated.

3dly, That it may be given with ſafety at all periods of the diſeaſe; whether in the remiſſion or exacerbation (*d*).

4thly, That, when it is rejected by the ſtomach, opium will in all caſes make it ſit quietly, and in ſufficient quantity to ſtop the fever.

5thly, That,

(*c*) The decoction cannot be depended on.

(*d*) This becomes abſolutely neceſſary when you happen to be called too late: for after the third or fourth day, the fits are protracted ſo long, as to run into one another; and when this is the caſe, whoever waits for a complete remiſſion, will find himſelf wofully diſappointed.

5thly, That, in order to make a ſufficient quantity ſit on the ſtomach, or to prevent it from running off by ſtool, opium may be given in any moderate doſe (*e*), during any ſymptom, and at any ſtage of the diſeaſe, whether in the remiſſion or exacerbation.

6thly, That the true pucca fever, as far as my experience extended, was not cured by any means except the bark, after the patient had ſuffered three regular returns of the fit; and that by this medicine it was eaſily cured, after petechiæ had appeared all over the body.

I concluded my addreſs to Sir John Pringle, with obſerving, that altho' theſe were the principles which conducted my practice in the cure of the pucca fever, I did not mean to advance them as rules to which there could not occur any excep-

tion;

(*e*) That is to ſay, in as great a doſe as is almoſt ever given in any caſe as a medicine. I have often found it neceſſary to give three grains in 24 hours; and was once under the neceſſity of giving more than five. I begin with a moderate doſe, which is repeated or increaſed juſt as I find occaſion.

tion; but that I never had met with any myſelf, and now proceeded on them with as little concern as if none could exiſt.

With regard to that ſpecies of putrid fever, deſcribed by Sir John Pringle himſelf, and other authors of Europe, I have ſome reaſon to believe, that they might be brought to a more ſpeedy termination, by an early and vigorous exhibition of the bark: but at all events, I have no doubt that it may be given in every ſtage with ſafety; and that it checks the fever, and prevents the putrefaction from making any advances, whilſt we continue at the ſame time to evacuate the corrupted contents of the bowels, and to ſupply the ſyſtem with freſh ſtores of aceſcent nouriſhment.

I am ſtrongly inclined to ſuſpect, that all the nervous fevers I have ſeen in this country, were nothing more than putrid fevers in a lower degree. This ſuggeſtion, however, I leave to the examination of farther experience; and ſhall only obſerve, that in all ſuch fevers, I have ever found the bark no leſs ſafe than it is uſeful.

Very few caſes of rheumatic fevers have come under my care: but, in all of them, the fever returned by fits; in all there was a large ſecretion of bile; and they were all cured by evacuations and the bark, like other bilious intermitting fevers.

Beſides the pucca fever, there occur in Bengal many intermittents and remittents purely bilious, which require nothing more for their cure than early and plentiful evacuations; and in the upper and more healthy parts of the country, the pucca fever very ſeldom makes its appearance. Still retaining, however, the idea of this tendency in the pucca fever to run on to deſtruction in ſpite of every evacuation, I was from this impreſſion often miſled into a premature exhibition of the bark, long after I had left Calcutta, and was removed to a climate where fevers were attended with much leſs danger. But when ſuch miſtakes were committed, that is, where the neceſſary evacuations had not been premiſed, they were very ſoon diſcovered and corrected. The bark was either thrown up with the bile in the courſe of the remiſſion; or if it remained on the ſtomach, not having power to prevent the ſucceeding paroxyſm, it ſeemed to render that more uneaſy. Theſe were all the bad conſequences that ever enſued; and in ſuch caſes all that was required, was to repeat the common evacuations for a remiſſion or two longer (*f*).

Prompted by this anxiety, in the beginning of my practice, to remove the fever as expeditiouſly as poſſible, and encouraged by the ſafety and efficacy of the bark in the Pucca fever, I was alſo frequently urged to an early exhibition of it in intermittents and remittents attended with pain and obſtruction in the liver; and,

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(*f*) In the hot ſeaſons of the year, I have ſeldom or never had occaſion to deſiſt from giving the bark after I had once begun to exhibit it: but in the cold weather, I have found it neceſſary to continue evacuations much longer; and if the ſkin can be well opened, it renders the effect of the bark much more certain.

and, from obſerving its innocence in all ſuch caſes, I have been led to a practice which I have ſince found to be ſafe and ſucceſsful; and of which I ſhall now communicate the ſubſtance.

Firſt, In intermittent fevers, which, from the habit and conſtitution of the patient, his mode of living, his long reſidence in this country, ſymptoms of obſtruction, the ſituation or ſeaſon of the year, I have reaſon to ſuſpect is diſpoſed to continue for ſome time, and is not likely to yield eaſily to evacuations alone, after the ſtomach and bowels are cleared of bile (which is always produced during the fit in great quantities), it is my conſtant rule to give the bark as ſoon as poſſible; in general, ſo as to prevent the third fit; and in caſes where the diſorder is habitual and well known, even ſo as to prevent the ſecond; provided that a ſufficiently copious evacuation of bile downwards can be effected during the firſt fit, or early enough in the beginning of the firſt remiſſion to admit of its being exhibited in ſufficient quantity, to be of any uſe in averting or alleviating the next expected return. In caſes where a tendency in the fever to continue is much apprehended, even a ſlight pain in the liver does not prevent me from purſuing this practice; unleſs I find it increaſe to any conſiderable degree by taking the bark.

I follow this practice, 1ſt, becauſe the pain and other ſymptoms of obſtruction in the liver, which may appear, or be increaſed on taking the bark (*g*), are not to

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(*g*) The large ſize of the liver, its warm ſituation in the body, and the languid circulation and peculiar nature of the blood which paſſes through it, ſuggeſt the probability of relaxation, accumulation, obſtruction, ſtagnation, corruption, and irregular ſecretion in this organ, in warm climates. And when we conſider the pains and ſwellings that are actually perceived in it, in the majority of Europeans who reſide in this country, together with the diſeaſed diſcharges of bile to which they are almoſt univerſally ſubject, that probability approaches in every individual nearly to a certainty. This being the caſe, it does not appear altogether fair to load the bark with the blame of creating all the obſtructions and diſeaſes of the liver, that ſhow themſelves on the exhibition of this medicine. The reaſoning which infers that it does no.

to be conſidered of any conſequence, when compared with the ſuperior tendency, which a continuation of the fever has to produce theſe effects, by accumulating the blood in the viſcera during every return of the paroxyſm. 2dly, Becauſe, when any ſymptoms of obſtructions are preſent, they are much more eaſily and effectually removed after the fever is gone, and any harm that may ariſe from the premature exhibition of the bark is remedied with little trouble; whereas the miſtake of delaying it too long admits of no remedy whatever. 3dly, Becauſe, independent of every conſideration of danger attending each fit, it is a matter of conſequence to every perſon to avoid the repetition of ſo ſevere an attack. 4thly, Becauſe it is alſo of

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no more than bring to light latent diſeaſes of the liver already exiſting, is ſupported by the prevailing probability, that the livers of Europeans reſiding in this country are rarely to be found in a ſound ſtate. And as the bark, when taken by people whoſe livers are perfectly ſound, does not produce thoſe effects, and is not accuſed of generating obſtructions in the other glands of the body free of diſeaſe, the concluſion which is made to its diſcredit has no analogy in its favour.

of importance to preſerve the patient's ſtrength, in a country where weakneſs always diſpoſes to a relapſe, and is attended with many other bad conſequences. And, 5thly, Becauſe, by ſtopping the fever immediately, you ſecure the patient's life; a conſideration ſuperior, ſurely, to every other, againſt a great many accidents that are likely to happen at all times in the progreſs of this diſorder; particularly from the want of proper ſick-bed attendants (*b*), which has very often proved fatal in this country.

Secondly,

(*b*) For example, evacuations are abſolutely neceſſary after every fit, whether the bark is to be given or not. The medicine preſcribed for this purpoſe, at a certain hour of the night, is from ſome cauſe or other neglected altogether, or not adminiſtered in a proper manner. The patient perhaps cannot be prevailed on to take it; or perhaps it is rejected immediately by the ſtomach, and there is no perſon at hand to ſeize the opportunity of ſubſtituting another in its place; and perhaps a falſe report is made to the ſurgeon in the morning. From cauſes ſuch as theſe, the evacuation, which ſhould have taken place in the intermiſſion, being neglected, the bowels and circulation get loaded with bile; the ſtomach becomes ſo weak and irritable as to receive neither medicines nor nouriſhment; the fever re- turns

Secondly, All the arguments I have been advancing in favour of an early exhibition of the bark in intermittents, are equally applicable in the caſe of remittents, whether attended or not with ſymptoms of obſtruction. And as theſe diſorders are more rapid in their progreſs, and more dangerous; ſo is the neceſſity of this practice in proportion more urgent. I do not recollect to have loſt a ſingle patient in this diſorder ſince I have proceeded on this plan; and I attribute my ſucceſs, to the diſpatch and freedom with which I go thro' the firſt evacuations; to the particular care I always take to be certain that theſe have been properly effected; to my early,

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turns with double violence, and continues without intermiſſion; and in ſhort, is converted into a dangerous bilious remittent. This leads me to obſerve, that I am inclined to ſuſpect, that the bilious fevers of the Weſt Indies with the yellow ſkin and other terrible ſymptoms, deſcribed by auauthors, are owing to a neglect of plentiful and repeated evacuations downwards, in the very beginning. For wherever I have met with it here, it has univerſally proceeded from this cauſe. And on the other hand I have never ſeen in it in that form, where proper evacuations were obtained at the beginning.

early, and, if I may be allowed the expreſſion, even to my premature exhibition of the bark; to the perfect confidence I put in this medicine, when given in powder to a ſufficient quantity; and to the free uſe I have made of opium in order to effect this purpoſe. In two or three caſes of bilious remittents, where no evacuations downwards could be effected by any means, and where I began to ſuſpect a putrid tendency, I have ſaved the patient's life by preparing the ſtomach with a large doſe of opium, and throwing in, during its operation, a quantity of bark ſufficient to ſtop the fever.

Thirdly, In intermittents and remittents, attended with any conſiderable degree of inflammation on the liver or any other part, veneſection muſt be inſtituted freely, as well as other evacuations; and in many caſes bliſters are neceſſary. After which, if the fever ſtill continue, and be not likely to ſtop by proſecuting this plan, the bark is to be given without heſitation: For in all the partial determinations I have met with, I have ever found the fever do much more harm in one fit, than all the bark that is neceſſary to ſtop its return.

Theſe being my ſentiments with regard to the bark, the uſe and application of the facts we have eſtabliſhed reſpecting the influence of the moon in the cure of fevers may now be explained without any interruption, and in the following manner.

1ſt. When an intermittent of any kind appears towards the end of the intervals, the firſt object to be held in view is to put a ſtop to it, if poſſible, before the approach of the full and change: becauſe, as I have already obſerved, the paroxyſms then become more ſevere, of longer duration, and more difficult to cure; and will ſometimes continue ſo long as to run into one another, and aſſume the form of a remittent, and afford no convenient opportunity for exhibiting the bark during the whole of that period. And although evacuations alone will generally remove the fever in the intervals, this is ſcarcely to be expected during the full and change.

For the ſame reaſon, when intermittents appear at the beginning of the full or change, the ſame object muſt be held in view; otherwiſe we muſt not look for a ſolution of the fever till that period be at at end.

On the other hand, it is to be obſerved, that when intermittents make their appearance towards the end of the full or change, there is not the ſame occaſion for a haſty exhibition of the bark: becauſe there is a probability, if not of a ſpontaneous ſolution of the fever, at leaſt of an abatement of its ſeverity, upon the expiration of theſe periods.

And we may alſo proceed more at leiſure when intermittents make their attack at the beginning of the intervals: for we have then ſufficient time before us both for plentiful evacuations, and for the bark; ſhould it be requiſite before the approach of the next apprehended revolution.

3d. One of the moſt important advantages to be derived from an attention to this ſyſtem, is the mode ſuggeſted by it of ſecuring againſt relapſes. Theſe generally happen at the full and change; and no perſon who has had an intermittent can conſider himſelf in any degree ſafe at theſe periods, until he has perfectly recovered his ſtrength, and removed every ſymptom of obſtruction. It is therefore abſolutely neceſſary to watch theſe returns with the greateſt care; and in general the uſe of laxatives, and a few doſes of bark given a day before, and continued every day whilſt the period laſts, will prevent a relapſe. When theſe precautions prove ineffectual, and the patient, in ſpite of all his endeavours, neither recovers ſtrength, nor gets quit of the ſymptoms of obſtruction, we are then taught to remove him with all expedition to a climate where the influence of the moon is leſs perceivable, and leſs prejudicial, than it is in Bengal (*i*).

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(*i*) At Madras it is much leſs felt; and a removal to that ſettlement from Bengal is, in many caſes, almoſt a certain cure.

All the common occaſional cauſes of fever are to be avoided with more than ordinary care at the full and change; ſuch as expoſure to the ſun, full meals of animal food, or whatever heats and irritates; and in ſhort, exceſſes of any kind. When this ſyſtem comes to be more generally underſtood, I flatter myſelf that it will ſuggeſt many uſeful hints for the better management of all Britiſh ſoldiers and ſailors ſerving their country in warm climates; and particularly of the latter, whoſe diet might be regulated by an attention to theſe unhealthy periods, without neglecting at the ſame time the judicious regard that is paid at preſent to a proper variety and interchange of food.

4th. With reſpect to bilious remittents, they are to be conſidered as no other than quotidian and tertian intermittents, whoſe fits are protracted by bile retained in the bowels or taken up into the circulation, by obſtructions of the liver and ſpleen, by the influence of the moon, or ſome other cauſe; and in them, an attention to all the preſent circumſtances we have juſt now pointed out is ſtill more neceſſary, than in intermittents; in proportion as their progreſs is more rapid, their danger greater, and their management more difficult.

The tendency of ſuch fevers to attack and remit, during the full and change, at certain times of the day, which I cannot help conſidering as intimately connected with the relative poſition of the ſun and moon with reſpect to us at theſe particular periods, belongs to this place, and furniſhes many uſeful indications in the method of cure.

I have learnt, by long experience, that all laxative and purgative medicines, as well as injections, are very uncertain in their operation, and generally diſappoint, ſo long as any degree of fever is preſent: And tartar emetic itſelf, with all the management we are maſter of, will often at this time operate only on the ſtomach, and produce no effect whatever on the bowels. The period, therefore, at which fevers ſhow a tendency to remit, muſt be watched carefully, and purgatives muſt in all caſes be adminiſtered on the firſt ſigns of a remiſſion. And although theſe ſhould not ſhow themſelves diſtinctly, ſtill the uſual period of remiſſion is to be preferred for this purpoſe. At this time they will generally operate, and evacuate the bile, which is the firſt, and indeed an indiſpenſable requiſite in the cure of theſe fevers.

When antimonials are to be given, with a view of cutting ſhort the fever, or of relieving the ſtomach of bile, the ſooner they be exhibited, ſo much the better. But if the intention be to carry off the abſorbed bile by perſpiration, and to procure a more complete remiſſion; the period we have juſt pointed out is to be choſen, that the operation of the medicine may occur with the tendency of the fever to remit.

This period is alſo the proper ſeaſon for throwing in the bark; and it is often of the greateſt conſequence not to allow a moment of it to eſcape, but to begin with the earlieſt ſymptoms of its commencement, or, when theſe are not manifeſt, at the earlieſt hour at which in other caſes they generally begin to make their appearance.

Theſe hints will ſuffice to ſhow the general application of our obſervations reſpecting this period to the cure of remittent fevers. On many occaſions, however, circumſtances are ſo urgent, that we are glad to ſeize the moment that is in our power for the exhibition of theſe medicines, without regard to any period whatever.

5thly, Putrid, nervous, and rheumatic fevers, are all, in this country, equally under the influence of the moon; and in all, our attention to theſe obſervations will be of the greateſt uſe, both in treating them when preſent, and in preventing relapſes.

6thly, My experience in the inoculation of the ſmall-pox is confined to a ſmall number of caſes; but from the few obſervations I have been able to make I am perfectly ſatisfied, that the full and change of the moon interfered with the eruption, and increaſed the fever to a dangerous degree. I have therefore determined to avoid this accident in future, by inoculating on the ſecond or third day of the full and change; ſo that the eruptive fever may always happen in the intervals. And I have no doubt that, on farther experience, this obſervation will become a matter of ſerious attention in the practice of inoculation, and afford many uſeful indications in the treatment of this diſorder when caught in the natural way (*k*.)

(*k*) I have long obſerved, that the ſecretion of bile is increaſed at the full and change of the moon, in many caſes where there is no fever. I have alſo obſerved, that whenever bile is taken up into the circulation, all wounds, ſores, boils, eruptions, the gums of children teething, rheumatic pains, &c. are remarkably inflamed and irritated by it; and I have farther obſerved, that all theſe complaints are remarkably inflamed and irritated at the full and change of the moon. From theſe premiſes I have been led to conclude, that the bile ſecreted at this time in greater quantity than uſual, may be the cauſe of the irritation and inflammation I mention. And

7thly, With regard to head-aches, tooth-aches, inflammations of the eyes, aſthmas, pains and ſwellings of the liver and ſpleen, fluxes, ſpaſms, obſtructions in the bowels, complaints in the urinary paſſages, eruptions of various kinds, and a great many more which return periodically with the moon, whether attended with fever or not, the cure entirely depends on a conſtant attention to theſe revolutions. By every ſucceeding return of ſuch complaints, the parts affected grow weaker and weaker, more liable to relapſe, and more difficult to cure. On the other hand, by preventing each return, the parts have a longer interval for

indeed this concluſion ſeems to be confirmed in a great degree by the good effects of purging in all ſuch caſes, and by the quantities of bile that are then carried off. Calomel, above all other medicines that I am acquainted with, poſſeſſes the power of carrying away the ſlimy and tenacious bile, with which the bowels are ſo apt to become loaded in this country. Query, May not the good effects of preparing for the ſmall-pox be owing to the evacuation of all kinds of acrid bile, previouſly to the infection? And may not the mercury which is given on this occaſion, by promoting a freer circulation in the liver, produce a more recent and leſs irritating bile?

gaining ſtrength, become leſs ſubject to relapſes, and at laſt recover their former tone. Therefore, when ſuch complaints do not originate from a diſeaſed liver, a proper attention to regimen and to the ſtate of the bowels, a judicious derivation from the part affected, and a timely exhibition of the bark before the approach of the lunar revolution, and during their continuation, will in general ſucceed. But it is to be remembered, that ſuch periodical complaints, in almoſt every caſe, are connected with a diſeaſed liver, which is beſt cured by mercury (*l*); and the bark is

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(*l*) Long neglected obſtructions of the liver generally terminate in dropſies; which, although far advanced, I have always found curable by a mercurial courſe, and other neceſſary attentions; provided that the diſorder was accompanied by a tolerably ſmart intermittent or remittent fever: and the only caſes in which ſuch a courſe failed were where there was no ſuch fever to be obſerved. In ſuch hydropical caſes attended with a fever, the tranſition from the full or change to the intervals is a very critical period, and often brings on a free diſcharge of urine: eſpecially if the blood be previouſly loaded with mercury, and a determination made exactly at this juncture to the kidneys by the exhibition of diuretics, ſuch as tincture of can-is nevertheleſs to be given at the full and change in ſuch quantity as to prevent relapſes.

8thly, As an attention to the ſyſtem we have been endeavouring to explain will teach the phyſician how to foreſee and provide for the various occurrences that are likely to happen in fevers; ſo it will enable him, not only to explain to others in a rational manner the paſt and preſent phenomena, but alſo to predict future events: a convincing proof of real ſcience, and a certain ſource of reputation and confidence to him, and of inexpreſſible ſatisfaction and eaſe to his patient.

Having thus found, by an application of the principles eſtabliſhed in our firſt pro-

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cantharides, ſquills, and alkaline ſalts. It would be an inveſtigation no leſs inſtructing than curious to aſcertain the exact dates of all dropſies cured by ſudden and unexpected diſcharges of urine; for I cannot help ſuſpecting, that moſt of theſe, as well as the ſudden and unexpected ſolutions of fevers, would be found connected, in a ſtriking manner, with the critical period I mention.

propoſition to the treatment of fevers, that they are not only uſeful in aſſiſting us to CURE, but alſo to PREVENT theſe diſorders, the truth of our ſecond propoſition follows of courſe.

ON THE

INFLUENCE OF THE MOON IN FEVERS.

PROPOSITION III.

THE INFLUENCE OF THE MOON IN FEVERS PREVAILS IN A SIMILAR MANNER IN EVERY INHABITED QUARTER OF THE GLOBE; AND CONSEQUENTLY, A SIMILAR ATTENTION TO IT IS A MATTER OF GENERAL IMPORTANCE IN THE PRACTICE OF MEDICINE.

BEING by no means prepared to ſupply the comprehenſive induction that is requiſite to eſtabliſh this propoſition on an unexceptionable or certain foundation, I muſt therefore proceed by a method leſs direct; and which, although it may not afford the ſame degree of certainty, may however, perhaps, anſwer the purpoſe of drawing the attention of the Faculty in other parts of the world, to a ſubject which really ſeems to deſerve it.

By my own experience and obſervations, I know that the influence of the moon, at the full and change, prevails in fevers from the 13th to the 26th degree of North Latitude; and we have certain accounts of it in Arabia and Perſia from the authority of the phyſicians of theſe countries. Hippocrates, who practiſed in Aſia and in Greece, and in latitudes ſtill higher than Arabia and Perſia, obſerved it and wrote of it 2000 years ago. And we have teſtimonies of its exiſtence in all the intermediate latitudes between Greece and Great Britain. Upon theſe grounds, it is not extending the analogy too far to conclude, that it prevails in every inhabited northern latitude (*m*). And theſe teſtimonies being alſo ſo many various proofs of its exiſtence in a great number of northern longitudes, we ſhall likewiſe

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(*m*) That is to ſay, as far north as the influence of the moon extends in the caſe of the tides.

wiſe venture to infer, that it prevails in every inhabited northern longitude. And in ſhort, (uniting the argument ariſing in favour of this concluſion from theſe particular inſtances, with that derived from the known univerſality of the moon's influence on the ebbing and flowing of the ſea), that it prevails over the whole northern hemiſphere. But having proceeded thus far, we are unavoidably led to advance ſtill farther on this analogical ground, and to conclude that the influence of the moon prevails equally in the ſouthern hemiſphere.

The univerſality of the moon's influence in fevers, all over the globe, being once admitted, it will follow, by a cloſer analogy, that its influence is exerted in a ſimilar manner at the ſame periods, and laſts for nearly the ſame length of time, viz. for ſix days at the full and change. It will follow alſo, that in all parts of the globe, the knowledge of this general law may be applied to the CURE and PREVEN-

TION of Fevers in the ſame manner as in India; and, conſequently, that AN ATTENTION TO IT MUST BE OF GENERAL IMPORTANCE IN THE PRACTICE OF MEDICINE.

ON THE

INFLUENCE OF THE MOON

IN FEVERS.

PROPOSITION IV.

THE WHOLE DOCTRINE OF THE CRISIS OF FEVERS MAY BE EASILY EXPLAINED FROM THE PREMISES WE HAVE ESTABLISHED RESPECTING THESE DISORDERS AT THE FULL AND CHANGE.

IF the hiſtories and deſcriptions of fevers had been handed down to us by medical authors, with a minute attention paid to the date of every occurrence in the courſe of the diſeaſe, ſo as to have aſcertained in every caſe the relative ſituation of the moon, I am inclined to believe that we ſhould have found the abatement and final ſolution of fevers ſo much connected with the expiration of the periods of the full and change, that the truth of this propoſition would have appeared at a ſingle view, without further inveſtigation or argument. But that indiſpenſable requiſite in every ſpecies of hiſtory has been neglected. And on the other hand, modern practice leaving much leſs than formerly to nature, and putting a ſtop to fevers in the very beginning, or interrupting them in their natural courſe and termination, denies us that aſſiſtance which we might otherwiſe receive from daily obſervation. I can therefore do nothing more than invite the attention of future obſervers to this curious and important ſubject, by aſſuring them, that I am fully convinced myſelf of the truth of this concluſion, from my own experience; and that in almoſt every caſe where I have had an opportunity of attending to fevers at the period when the full or change expired, and the intervals commenced, I have obſerved almoſt invariably, either ſome ſymptom of the abatement of the fever, or a perfect ſolution.

This obſervation, which was firſt made on common bilious and rheumatic fevers, firſt led me to conclude, that the tranſition from the full and change to the intervals is *a favourable critical period* in fevers, and that all the days of the interval are alſo *favourable.* And as I have likewiſe been able to obſerve almoſt invariably an increaſe of the fever upon its paſſing from the intervals into the full and change, and during the continuation of theſe periods, and frequently death; I therefore ventured to conclude, that the tranſition from the intervals to the full and change, is an *unfavourable critical period* in fevers; and that all the ſix days formerly deſcribed, are alſo *unfavourable:* in other words, that "*along with the* "*full and change of the moon there is* "*conſtantly recurring ſome uncommon or* "*adventitious ſtate or quality in the air,* "*which increaſes fever, and diſpoſes to* "*an unfavourable termination or criſis;* "*and that along with the intervals there* "*is as conſtantly recurring a ſtate or quality in the air, oppoſite to the former,* "*which does not excite, but diminiſhes* "*fever,*

"*fever, and diſpoſes to a favourable cri-*
"*ſis* (*n*)."

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(*n*) If this be true, beſides having eſtabliſhed theſe propoſitions, we ſhall have alſo approached by a very conſiderable ſtep towards a more intimate acquaintance with the more immediate cauſe of fever: for, by a comprehenſive ſyſtem of experiments and obſervations, we ſhall now be able to aſcertain the nature of that ſtate or quality in the air which cauſes ſo eſſential a difference at theſe periods. Such a ſyſtem would require a journal of every lunar day, containing a co-temporary record of fevers, and other diſorders of the human body in a ſtate of health; of the heat, moiſture, and weight of the atmoſphere, and various winds; of experiments and obſervations made on the ſtate of electricity and putrefaction; of the various revolutions of the ſun, moon, and ſtars; and of a great many other circumſtances that would be ſuggeſted occaſionally; in ſuch a manner as to aſcertain their relative ſtates and ſituations, and finally their connection as cauſes and effects. An undertaking of this kind would be more than enough for the conſtant employment of one man, and is far out of my reach. But it belongs to this ſubject, and is worthy of being remarked, that it is a fact eſtabliſhed in this country, on general obſervation, that the tendency of meat to ſpoil is much greater at the full of the moon than during the intervals. I know alſo from experience, that at all ſeaſons of the year the full and change ſeldom fail to produce, for ſome time, an uncommon calmneſs, heat, and cloſeneſs in the air; and, if I be not miſtaken, it is that calmneſs, heat, and cloſeneſs, which lay the foundation of the winds which prevail ſo much about theſe periods.

The hiſtories of fevers delivered to us by Hippocrates being deficient in the eſſential requiſite of date; and his account of criſes ſo much mixed with a particular theory of his own concerning the original ſtadia of fevers, and alſo with ſome vague and unphiloſophical ideas reſpecting the aſpects and conjunctions of benign and malevolent planets, any attempt to reaſon on what he has delivered to us on this ſubject, would be a work of mere conjecture, and afford but little ſatisfaction. It is more to the purpoſe to ſay, that ſince I have given my attention to it, I have met with no turn or termination in bilious, rheumatic, or nervous fevers, which I have not been able to explain to my own ſatisfaction on this ſyſtem; that I have alſo been able to predict their turns and terminations with much certainty; and that the duration of ſuch fevers is not limited to any fixed *critical period* depending on odd or even days, but is conſtantly connected with the *favourable and unfavourable critical periods* I have juſt de-

deſcribed. And whenever theſe periods interfere with the eruptive fever of the ſmall-pox, I am convinced from experience that they are to be conſidered in no other light.

From what has been already obſerved on the ſubject of the putrid or pucca fever of Bengal, I have no doubt in determining that their *favourable and unfavourable critical periods* are the ſame with thoſe of the bilious, rheumatic, nervous fevers; and of the eruptive fever of the ſmall-pox. But to reconcile to this doctrine the putrid fevers deſcribed by Sir John Pringle, Mr Tiſſot, and Dr Hilary, terminating regularly in 14, 17, and 19 days, is a matter of great difficulty. And I muſt here once more regret, that all theſe hiſtories are materially deficient in being deſtitute of every kind of date; and that I am again reduced to the neceſſity of advancing with the faint and partial light of my own experience.

In the courſe of my endeavours to account for theſe facts, which at firſt ſeemed to eſtabliſh a limited duration to particular fevers, independent of any *favourable or unfavourable critical periods*, and therefore to militate ſtrongly againſt our preſent theory, I was led into the following train of conjecture.

In the caſe of putrid fevers, continuing 19 days, I ſuppoſed that there muſt have been a ſtrong putrid tendency in the habit, and that the febriferous (*o*) influence of the air which prevails at the full and change, co-operating with this tendency at theſe periods, had the power of producing a fever on the ſecond day from their commencement; and that before means could be uſed to ſtop or correct this diſpoſition in the patient's habit, the fever con-

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(*o*) If it be confirmed by farther experience that this febriferous influence is conſtantly connected with an increaſed tendency to putrefaction, which there is ſome reaſon to ſuppoſe, from the fact related in the preceeding note, the account we have given of the various duration of putrid fevers will be rendered ſtill more probable and intelligible; and be applicable to every fever of this kind, whether longer or ſhorter than thoſe which we have here ſpecified as examples to illuſtrate the theory of the whole claſs.

continued to run on through the firſt full or change, and ſucceeding interval, and alſo through a ſecond full or change; but that the putrid tendency being now in ſome degree overcome by medicine, and at the ſame time the febriferous influence of the full or change removed by the arrival of the ſecond interval, a criſis of conſequence immediately took place at this juncture, juſt about 19 days from the firſt attack.

In the caſe of putrid fevers continuing only 17 days, I ſuppoſed that in them the putrid tendency of the habit was ſomewhat leſs at the beginning than in the former caſe; and that the febriferous influence of the full or change, had not power to excite a fever until the fourth day of the period, when the putrid tendency was farther advanced; that the fever continued to run on during the remaining days of that full or change, thro' the ſucceeding interval, and alſo through another entire full or change, in the ſame manner as the fever of 19 days; and that at laſt, from the concurrence of the ſame cauſes, it terminated critically, immediately on the commencement of the ſecond interval; juſt about 17 days from the firſt attack.

And laſtly, in the caſe of putrid fevers continuing only 14 days, I ſuppoſed that the putrid tendency in them being ſtill leſs than in thoſe of 17 days, the febriferous influence of the full or change had not power to excite a fever until the very cloſe of the *period* when the putrid tendency was ſufficiently advanced, or towards the beginning of the interval; during which interval, and the whole of the ſucceeding full or change, it continued to run on, and at laſt from the concurrence of the cauſes we have juſt explained, terminated critically, immediately on the commencement of the ſecond interval, juſt about 14 days from the firſt attack.

Since I became poſſeſſed of theſe ſentiments regarding criſis, and the above theory of putrid fevers, I have had an opportunity of meeting with only four or five caſes of this kind. In one of theſe the fever continued exactly ſeventeen days, and terminated completely and finally on the commencement of the ſecond interval; a circumſtance which afforded me no ſmall ſatisfaction, as I had predicted the criſis, on the theory juſt explained, at that very juncture, and was looking for it with anxious expectation. In the other caſes I had not an opportunity of aſcertaining the beginning of the fever, nor conſequently the exact time of its duration; and a perfect ſolution did not take place, as I had expected on the approach of the ſecond interval; but in all of them the diſorder took ſo *favourable* a turn at this *period*, that it might be called without impropriety, a criſis of the fever.

Whether I have hit on a juſt explanation of the cauſe of that variety which apapears in the duration of theſe period fevers, is a queſtion which I ſhall leave to the deciſion of farther experience. But I will not ſcruple to pronounce, even from theſe few examples, that they, as well as the other fevers of which we have already ſpoken, have their *favourable* and *unfavorable critical periods*; and that theſe are no other than what we have already deſcribed, viz. the full and change of the moon.

The detail which I have brought to ſupport my opinion, reſpecting *favourable* and *unfavourable critical periods* is now only defective in examples of inflammatory fevers; and although I cannot ſay, that I have had an opportunity in this country of making obſervations on any that could be eſteemed purely and ſolely inflammatory; yet as I have ſeen the effects of theſe *periods* in partial inflammations, in fevers attended with inflammatory ſymptoms, and in every other kind of fever, I ſhall not conſider them as any exception to the general rule.

Whilſt we employ this doctrine *of fafavourable* and *unfavourable critical periods* to explain the different criſis of fevers, the ancient theory of *concoction* muſt of courſe fall to the ground. But there is no neceſſity on that account for rejecting the idea of a *morbid matter*, which in many inſtances certainly exiſts, and which we conceive may be perfectly reconciled with our preſent ſyſtem on the following terms.

1*ſt*, That in bilious and inflammatory fevers, which we know may be ſtopt in the beginning, or at any other ſtage, a *morbid matter*, if any exiſt, has ſo ſmall a ſhare in determining the criſis, that it is not to be regarded in practice; and that in all ſuch caſes *the favourable and unfavourable critical periods* demand our principal attention. The termination of nervous and rheumatic fevers ſeems alſo to be much more under the dominion of theſe *periods*, than any internal conſtitutional principle that I have been able to diſcover (*p*); and therefore, come under the ſame rule.

2*dly*, That

(*p*) If it ſhould be determined by future obſervation, that moſt of the fevers called Nervous, differ from the Putrid only in degree, it will probably be found alſo, that their progreſs and duration are affected by the *ſavourable* and *unſavourable critical periods* in a ſimilar manner.

2dly, That in the ſmall-pox and meaſles, and other ſuch diſeaſes, the duration of the fever ſeems to be chiefly determined by the peculiar nature of the infection: but that great attention is alſo to be paid to the *favourable*, and eſpecially *the unfavourable critical periods*, which may aggravate the ſymptoms, interrupt the natural progreſs of the fever, and protract it beyond its uſual length.

3dly, That in putrid fevers there is a putrid tendency in the habit, to overcome which a conſiderable ſpace of time is often requiſite; ſometimes more, ſometimes leſs, depending on the degree to which it is advanced, and perhaps other circumſtances: and that their apparent imitation, on certain occaſions, to a fixed duration, ariſes from the influence of *the favourable* and *unfavourable critical periods*, exerting itſelf in the manner already deſcribed.

4thly, That the natural tendency of the conſtitution, with the concurrence of other occaſional cauſes, may produce fevers, in the intervals, independent of any aſſiſtance from the febriferous influence of the moon: and when fevers of any kind begin and terminate in the intervals, the effects of the *unfavourable critical periods* will not interfere in ſuch caſes; and muſt not therefore be expected.

5thly, That when the internal cauſe of fever is very powerful, and the ſymptoms run extremely high, the effects of *the favourable* and *unfavourable critical periods* may not be obſerved, although their influence in ſuch caſes neverthelesſs ſtill continues to be exerted.

To ſum up the whole: It appears, that, by eſtabliſhing the exiſtence of *favourable* and *unfavourable critical periods*, we have acquired the knowledge of a principle which is uſeful in the CURE and PREVENTION of fevers: and which alſo teaches to PREDICT and EXPLAIN their various CRISES on grounds that are conſiſtent and ſatisfactory. It is, therefore, unphiloſophical to ſearch for any other; and our fourth propoſition muſt remain unſhaken until it be refuted; not by the bare diſſent of one or two, who may not have directed their attention expreſsly to this ſubject, and by whom the very phenomena upon which the whole ſyſtem is built may have paſſed unnoticed or uncollected; but by the united experience and opinions of many future accurate and intelligent obſervers.

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